Title: Psychological Distress in French College Students: Demographic, Economic and Social Stressors. Results from the 2010 National Health Barometer

Authors:

Thomas Saias (saias.thomas@uqam.ca)
Enguerrand du Roscoät (Enguerrand.DU-ROSCOAT@inpes.sante.fr)
Laurentine Véron (apsytude@gmail.com)
Romain Guignard (Romain.guignard@inpes.sante.fr)
Jean-Baptiste Richard (Jean-baptiste.richard@inpes.sante.fr)
Stéphane Legleye (stephane.legleye@ined.fr)
Fanny Sauvade (apsytude@gmail.com)
Viviane Kovess (vkovess@gmail.com)
François Beck (Francois.beck@inpes.sante.fr)

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Author's response to reviews: see over
**Manuscript title:** “Psychological Distress in French College Students: Demographic, Economic and Social Stressors. Results from the 2010 National Health Barometer”, by Thomas Saïas et al.

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**Answers to the Reviewers’ comments with modifications**

*we would like to introduce to the text.*

**Reviewer 1**

<table>
<thead>
<tr>
<th>General comments</th>
</tr>
</thead>
<tbody>
<tr>
<td>The writing is acceptable. However, there are minor mistakes (for instance the MH-5 when first mentioned, both in the abstract and the text, should be written with full title and than by abbreviation - Mental health subscale of the SF-36); English should be a little improved (&quot;such as burnout&quot; instead of &quot;among burnout&quot;; &quot;especially&quot; instead of &quot;specially&quot;; &quot;it has also been shown a direct link between PD and social participation&quot; should be changed into &quot;a direct link between PD and social participation has been also shown&quot;). Answer and proposed modifications We have taken this remark into account and here are our proposed changes for a 300-word abstract.</td>
</tr>
</tbody>
</table>

**Modifications included in the revised manuscript**

The whole paper has been checked for English and all of these modifications have been taken into account.

**Reviewer 2**

<table>
<thead>
<tr>
<th>Introduction</th>
</tr>
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<tbody>
<tr>
<td>As noted above, this section needs to be revised to provide more information about the research available on psychological distress among students in different populations. A more thorough discussion of reasons for possible differences in these findings, particularly given the different outcome measures used across studies, is warranted.</td>
</tr>
</tbody>
</table>

The authors should more clearly identify reasons for including the variables they selected in their analyses. The manner in which previous findings are discussed is too general (e.g., grouping life events, psychological symptoms, financial stress and year in college into a single summative sentence). Given the number of variables under study, it is important to ensure that they are all important to assess, particularly given the possibility of committing a Type I error. |

**Modifications included in the revised manuscript**

We thank the reviewer for giving us the opportunity to rewrite the introduction of this paper. This introductive section has been substantially modified as presented in the revised
manuscript. We hope that the focus on the specific variables we chose to use will be suitable to the expert.

## Methods

*The authors should note how informed consent was obtained.* *(Minor Essential Revision)*

**Modifications included in the revised manuscript**

“In informal consent was obtained when starting the telephone interview, following guidelines from the French commission on data privacy and public liberties (CNIL).”

*The authors should provide some context for many students there are in France, and thus, what percentage of this population they obtained with their 946 participants.* *(Discretionary Revision)*

**Modifications included in the revised manuscript**

“The year of the survey, 2.3 millions persons were college students in France, which represents a 0.04% sampling rate”.

*The authors note that individuals in “collective dwellings” were not included in analyses. They should specifically note whether this excluded college dormitories or other common student residential options, as such an exclusion would notably impact the generalizability of their obtained sample.* *(Minor Essential Revision)*

&

*The procedure by which participants in a given dwelling were selected should be explained more clearly, and not just refer to the procedure used by another researcher.*

**Modifications included in the revised manuscript**

“This survey was based on a two-stage random sample of 27,653 French people aged 15 to 85 years speaking French and living in metropolitan France. Residents of collective dwellings, hospitals and institutions were excluded from the target population. This included college dormitories or other common student residential options, but this did not notably impact the generalizability of the obtained sample because a vast majority of them could be reached by their parent’s household or by their mobile phone.

The first step sampling was household selection (by phone number), then an eligible subject was selected to answer the questionnaire. This selection was made by using the method...
proposed by Leslie Kish: everybody in the household was first listed, then one of them was randomly selected by a CATI computing procedure.”

*The authors should justify why they are using a cut-off of general self-reported psychological symptoms. This issue is important given concerns about dichotomous diagnostic criteria, particularly when looking at diffuse symptoms. It is unknown if this level of symptomatology reflects functional impairment (required for psychological diagnoses), and thus, represents the broader construct of severe distress the authors represent that they assessed. Of note, the impact that this choice may have had on their results (e.g., lower prevalence of PD than expected) should be included in the Discussion.*

**Modifications included in the revised manuscript**

No modifications were included. While the MH5 can be useful in research and screening practices in clinical practice, it does not include functional impairment and thus is not a psychological diagnosis. A cut-off supported by the literature was established to facilitate comparisons with other studies.

*The manner in which variables are labeled (e.g., “isolation” is whether “respondents lived alone in their own home”) is misleading at times. The authors must make clear throughout this section, as well as the rest of the manuscript, what exactly was, and was not, assessed. There are a number of limitations of the assessed variables (e.g., sexual violence was assessed using two broad, non-behaviorally-specific questions, social participation is framed as social support broadly, while opportunity of social contacts with friends and family is ignored).*  

**Modifications included in the revised manuscript**

The manuscript has been checked for these misleading vocabulary differences

*The authors should be clear whether the life experiences variables assessed (e.g., non-sexual violence, serious illness of a parent) were assessed using close-ended questions. The wording of such questions may also be helpful, given that sexual violence was assessed in such a broad manner. (Minor Essential Revision)*

**Modifications included in the revised manuscript**

The specific questions were included in the manuscript

*It is unclear why life experience variables were assessed before the age of 18, but not after. (Discretionary Revision)*

&
The changing timeframe for the life event variables should be made clearer, and discussed with respect to their limitations (i.e., lifetime sexual violence, non-sexual violence in the past year, life events prior to age 18). (Minor Essential Revision)

Modifications included in the revised manuscript

In the survey, those events were restricted to those happened before 18 because their impact much depends on the period of life in which they occur e.g.:


The manner of weighting the sample should be explained more clearly, and whether or not a stratified sample was used should be mentioned. (Discretionary Revision)

Modifications included in the revised manuscript

“They were then adjusted to calibrate to the target population structure (the 2008 French population structure issued from the National Institute of Statistics and Economic Studies) according to age, gender, region, level of urbanisation and educational level.”

Results

The inclusion of information about debt should be qualified with respect to whether or not students generally take out loans to pay for school in France. Given that this experience is relatively commonplace for many students in the US, the discussion of students in debt in this population needs further contextualization. Of note, though, the use of perceived personal economic strain is an interesting and valuable variable in this regard. (Minor Essential Revision)

Modifications included in the revised manuscript

In the discussion

The authors note a result with respect to “contact with friends or family in the last 8 days…” when, in fact, it was assessed within the past week. (Minor Essential Revision)
Modifications included in the revised manuscript
Text was modified accordingly

# Discussion

As described above, this section needs substantial revision to more accurately portray the obtained results. The authors are too broad in the manner in which they discuss their findings. While the results are interesting, they must speak to the actual constructs that were assessed (e.g., engagement in leisure time activities) and not the broader variables that may or may not be tapped by these operational definitions (e.g., social support more broadly).

Modifications included in the revised manuscript

We thank the reviewer for giving us the opportunity to clarify our discussion, with regards to the actual findings that were given in the results section.

The end of the discussion has been rewritten as follows:

“Finally, as expected we found that low participation to social activities (The item was: “In the last week, have you had group or leisure activities such as meetings, religious activities, associative activities, going to theatre, practicing sports, going to parties…?”) was related to higher level of PD, starting with students declaring they didn’t participate to any group or leisure activities (OR = 4.1) to students declaring they participate a little (OR= 2.3), compared to those who declared to participate a lot.

Participation to social activities is thus the highest odd for PD that we observed in our study. This finding calls for the development of actions aiming at promoting group of leisure activities, as a general preventive instrument, among students populations.”

The non-significant results and results that are inconsistent with the previous literature (e.g., sexual violence, engagement with family and friends) should be addressed. The relations between these unexpected findings should be discussed in the context of the broader literature in this area, as well as the study’s limitations (e.g., limited assessment of these variables).

Modifications included in the revised manuscript

“While we have found out that, consistently with the literature, violence in the last year was highly related to psychological distress, sexual assault in life was not significantly associated to our dependent variable, although a clear trend was identifiable in the bivariate analyses (participants who had been victim of sexual violence in life being 22.7% to declare PD comparing to 13.5% in those who hadn’t been victim of such assaults ; F = 3.7 ; p=.05), this link becoming non-significant while controlling for other variables. Co-variation with other
independent variables (gender and social isolation) could partially explain this finding. The limited number of variables used to confine the process by which life-time sexual violence impacts the expression of distress in students does not allow us to draw firm conclusions from this study.”

*The discussion of major choice as a factor in PD is confusing as written (e.g., “…only first-year students from public universities in which the conditions of education, social and professional opportunities are more uncertain than in engineering or business schools that are included in our sample.”) This variable and its implications should be addressed more clearly.*

**Modifications included in the revised manuscript**

Consistently with previous literature, we chose to include this variable in our analyses. Besides studying law (as a protective factor), and medicine, no other specific outcome required further analysis, given our results.

*The authors have some interesting ideas with respect to preventing PD among students by mobilizing social support, and the inclusion of possible interacting variables (e.g., female gender, economic strain), is useful. However, this discussion is too long and far-reaching for the results that were actually obtained, particularly given that only one of the social support-related variables was significant in the study analyses. This discussion should be condensed (it is brought up multiple times throughout the Discussion) and discussed within the context of what was actually found.*

**Modifications included in the revised manuscript**

The discussion was shortened, accordingly to the request of the reviewer.

*The comparison of this sample to unemployed individuals is surprising and does not seem warranted, given the notable differences between these groups. (Discretionary Revision)*

**Modifications included in the revised manuscript**

This precision was deleted